

**Comment on the proposed conservation of the specific name *Boccardia proboscidea* Hartman, 1940 (Annelida, SPIONIDAE)**  
(Case 3520; see BZN 67: 203–210)

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The application by Radashevsky & Harris (BZN 67: 203–210) asked for conservation of the specific name *Boccardia proboscidea* Hartman, 1940, used for a widely dispersed mudworm (family SPIONIDAE) described from California and requested that all previous type designations for *B. proboscidea* be set aside in favour of a neotype. Part of their rationale for designating a neotype was based on the fact that while Hartman (1940) stated that the holotype was deposited at the United States National Museum (USNM), the vial Hartman sent to the USNM contained 12 specimens.

However, designation of a neotype would limit the name-bearing types of *B. proboscidea* to one specimen and deprive all the other specimens of this status. Such an act would contradict Hartman's concept of type specimens which was based on the 1st and 2nd editions of the ICZN. Prior to 1999 the Code did not require a type specimen. Only after 1999 were holotype or syntypes required to be designated for any newly-described species-group taxon (Articles 72.2, 72.3 of the current Code).

We do not know exactly why Hartman listed the multiple-specimen type lot of *B. proboscidea* as 'the holotype'. In a letter dated 19 February 1937 to Dr Waldo Schmitt, Curator of Invertebrates, USNM, she said 'I have sent off to you today, eight vials containing polychaetous specimens designated as holotypes'. In his return letter dated 27 February 1937, Dr Schmitt replied 'In a few cases you had more than one worm in a bottle marked holotype. Of course, we shall select the nicest looking one for the holotype, but in the future it would be better if you were to specifically designate one species [sic; *lapsus* for specimen] of a lot as holotype either by tying on a bit of thread or else putting it in a separate vial' (excerpts of the Hartman-Schmitt correspondence provided courtesy of the Smithsonian Institution Archives). Besides *B. proboscidea*, other spionid species with multi-specimen 'holotypes' are *Polydora amaricola* Hartman, 1936 (USNM 20214, 5 specimens, status listed as type in the USNM catalogue), *Polydora brachycephala* Hartman, 1936 (USNM 20215, 4 specimens, status listed as syntype in the USNM catalogue), *Pygospio californica* Hartman



1936 (USNM 20219, 4 specimens, status listed as type in the USNM catalogue), *Rhynchospio arenicola* Hartman, 1936 (USNM 20221, 4 specimens, status erroneously listed as paratype in the USNM catalogue), and *Streblospio luticola* Hartman, 1936 (USNM 20220, 15 specimens, status erroneously listed as paratype in the USNM catalogue). These problematic cases will be described in an upcoming catalogue of types of the spionid polychaetes deposited in North American museums (Harris & Radashevsky, in preparation).

Joint work and personal collaboration with Olga Hartman by one of us (KF) and extensive study of Hartman's personal papers preserved in the LACM by another (LH) unequivocally shows that Hartman believed a series of type specimens better represented a new species than a single holotype. Single specimen holotypes were designated by Hartman mainly when only one individual was available for examination. We therefore assume that her designation of multiple-specimen type lots was due to Hartman's personal concept of types. Why Hartman called these multiple-specimen lots holotypes instead of syntypes, as she did in some other cases, remains unknown.

Hartman stated in several early papers that she deposited the holotypes of newly described species in the United States National Museum in Washington, D.C. and split the paratypes between the Zoological Museum of the University of California, Berkeley (where she received her Ph.D.) and the California Academy of Sciences, San Francisco. Around 1943 the bulk of the University of California polychaete collection, including type material, was given to the Allan Hancock Foundation, University of Southern California (AHF). As previously mentioned by Radashevsky & Harris (BZN 67: 203–210), there is evidence in Hartman's personal papers and collection labels (LACM-AHF Polychaete Collection archives, unpublished) to show that LACM-AHF POLY 1226 was considered by Hartman to be a type lot for *B. proboscidea*. An early Allan Hancock Foundation type inventory made by Hartman includes '*Boccardia proboscidea* Hartman AHF no. 117 [cotype]'. The specimens in this lot and in the 'holotype' lot (USNM 20217) were all collected by Hartman on 4 July 1934, from vertical burrows in intertidal sandstone at Caspar, Mendocino County, California. Both lots have been examined by VIR and found to include specimens of the same species in good condition. Under Article 72.4.1.1 of the current Code ('For a nominal species or subspecies established before 2000, any evidence, published or unpublished, may be taken into account to determine what specimens constitute the type series.') these two samples represent valid type material of the species.

Article 75.5 of the Code was incorrectly used by Radashevsky & Harris (BZN 67: 205) to justify designation of neotype for *B. proboscidea*. It refers to a situation when 'the taxonomic identity of a nominal species-group taxon cannot be determined from its existing name-bearing type (i.e. its name is a nomen dubium), and stability or universality are threatened thereby'. As Hartman's types do exist in good condition with the problem being the uncertainty of which specimen is the holotype (i.e. the name-bearing specimen), we believe that the best solution about these types would be to leave them as syntypes. This would also be in agreement with what we feel was Hartman's original intent.

Consequently, we here suggest rephrasing the proposal by Radashevsky & Harris (BZN 67: 203–210) in the following manner:



13. The International Commission of Zoological Nomenclature is accordingly asked:

- (1) to use its plenary power:
  - (a) to rule that the name *proboscidea* Hartman, 1940, as published in the binomen *Boccardia proboscidea*, be given precedence over *californica* Treadwell, 1914, as published in the binomen *Polydora californica*, whenever the two are considered to be synonyms;
  - (b) to suppress the name *californica* Fewkes, 1889, as published in the binomen *Spio californica*, for the purposes of the Principle of Priority but not for those of the Principle of Homonymy;
- (2) to place on the Official List of Specific Names in Zoology the following names:
  - (a) *proboscidea* Hartman, 1940, as published in the binomen *Boccardia proboscidea* and as defined by syntypes USNM 20217 and LACM-AHF POLY 1226, with the endorsement that it is to be given precedence over *californica* Treadwell, 1914, as published in the binomen *Polydora californica*, whenever the two are considered to be synonyms;
  - (b) *californica* Treadwell, 1914, as published in the binomen *Polydora californica*, with the endorsement that it is not to be given priority over *proboscidea* Hartman, 1940, as published in the binomen *Boccardia proboscidea* and as defined by syntypes USNM 20217 and LACM-AHF POLY 1226, whenever the two are considered to be synonyms;
- (3) to place on the Official Index of Rejected and Invalid Specific Names in Zoology the name *californica* Fewkes, 1889, as published in the binomen *Spio californica* and as suppressed in (1)(b) above.

**Comments on the proposed designation of a neotype for the nominal species *Chionobas chryxus* Doubleday, 1849 (currently *Oeneis chryxus*; Insecta, Lepidoptera, NYMPHALIDAE)**  
(Case 3495; see BZN 67: 121–128)

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I strongly disagree that stability is served by use of the plenary power to suppress the validly designated lectotype of *Chionobas chryxus* and replace it with a neotype.

1. The lectotype of *Oeneis chryxus* designated by Shepard (1984) represents the taxon as it has been understood since its description. The statement in the abstract of the petition ‘the original figure perfectly matches males of one of the two species into which the species was later divided’ suggests widespread acceptance that there are two species when actually the matter of this species division into two is based only on two papers in the same publication.

2. Only one paper has been published to date that utilises the nomenclature put forth by the authors in 2006 (Kondla, 2010) and that paper is by one of those authors.



3. The author of the petition claims that the lectotype is indeterminate, but this is a subjective matter not supported by the views of subsequent researchers. There is no reason to suppose that the male figured in Doubleday & Hewitson, apparently lost, is not the same species as the lectotype, possibly even collected with it, and such has been held to be the case since the lectotype was designated. Most researchers are completely unaware of the issue and it seems prudent to await serious consideration from a broad spectrum of naturalists before any decision requiring the plenary power is rendered.

4. It is my opinion that the designation of a neotype through the exercise of the plenary power is unwarranted in the face of what remains a very limited view. It does not stabilise an uncertain nomenclature because at this time there is no uncertain nomenclature.

### Additional references

Kondla, N.G. 2010. Section 2. Butterflies. Pp. 163–192 in Pohl, G.R., Anweiler, G.G., Schmidt, B.C. & Kondla, N.G. An annotated list of the Lepidoptera of Alberta, Canada. *ZooKeys*, 38: 1–549.

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There is no ‘exceptional need’ for a neotype of *Chionobas chryxus*. The petitioner claims that there are two species of ‘*Oeneis chryxus*’ occurring in Colorado and elsewhere in the Rocky Mountains of western North America. This hypothesis was recently proposed (Scott, 2006), and has never been tested through rigorous morphological study or molecular techniques. Subsequent authors dealing with the North American fauna have not followed Scott’s nomenclature (with the single exception of Kondla (2010)). Much of the wording in Scott’s petition portrays as ‘fact’ concepts that have never been corroborated by detailed research. Many statements presented as fact about ‘two species’ in the southern Rocky Mountains on pages 125–127 of the petition are debatable, and some are erroneous. My own experience with *Oeneis* in Colorado (where I grew up collecting them regularly, including the same populations Scott has based his hypotheses upon), as well as current insight gleaned from curating 3,376 specimens of the *Oeneis chryxus* complex in the collections of the McGuire Center for Lepidoptera and Biodiversity, Florida Museum of Natural History, University of Florida [MGCL], suggest that Scott has badly misinterpreted the actual patterns of geographic variation in *Oeneis chryxus*.

There seem to be two taxonomic entities within *Oeneis chryxus* in Colorado, but my preliminary analysis of MGCL material indicates only one species is likely to be present. This same analysis indicates that the high-elevation entity *O. chryxus altacordillera* Scott from Colorado does not occur to the north in the Rocky Mountains of Wyoming, Montana and Alberta. In this region, only one taxon is



present, which has always been regarded as *Oeneis chryxus*. *O. chryxus altacordillera* does not occur in Alberta (none of 155 specimens examined from Alberta in MGCL could be considered *altacordillera*), and its occurrence in Montana and Wyoming is doubtful. Thus, there should be no confusion over the identity of Shepard's (1984) lectotype for *Chionobas chryxus*, very probably from Alberta.

Much of Scott's argument for the need of a neotype is based on the hypothesis that females of *O. chryxus* are not useful for identifying subspecies (or sibling species as claimed by Scott). In my experience, this is simply not the case. Females of *O. chryxus* demonstrate as much geographic variation as males, and are useful for identifying subspecies-level taxa, including *altacordillera* (as defined by Scott). Most importantly, all authors prior to Scott (Case 3495, who suggested Wyoming) agree that Shepard's (1984) lectotype female likely originated in the Rocky Mountains of Alberta.

My analysis of the *Oeneis chryxus* group, together with recent literature, leads me to believe that Scott's hypotheses about species-level relationships in the group are almost certainly incorrect. Most of the statements presented as facts about the supposed species diversity of the group in the southern Rocky Mountains are untested hypotheses, and have not been widely accepted in the recent literature. Most importantly, if Scott's *altacordillera* does not occur as far north as the Rocky Mountains of Alberta, there should be no confusion over the identity of any female *Oeneis chryxus* from this region or of Shepard's (1984) lectotype from the Alberta Rockies, so therefore there is absolutely no need for a neotype.

### Additional references

Kondla, N.G. 2010. Section 2. Butterflies. Pp. 163–192 in Pohl, G.R., Anweiler, G.G., Schmidt, B.C. & Kondla, N.G. An annotated list of the Lepidoptera of Alberta, Canada. *ZooKeys*, 38: 1–549.

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This case is based on the premise that the nominal species *Chionobas chryxus* can not be identified from its existing name-bearing type, and stability or universality are thereby threatened. With the exception of Scott (2006), in which *Oeneis calais altacordillera* was described, the identity of *C. chryxus* has remained virtually uncontested since it was named and figured in 1849. Kondla (2010), who co-authored portions of Scott (2006), is one of the few authors to subsequently employ the name *Oeneis calais altacordillera*. Based on recommendations from other lepidopterists, Pelham (2008) listed both *calais* and *altacordillera* as subspecies of *chryxus*. Holland (2010) also treated *altacordillera* as a subspecies of *C. chryxus*, which he characterised as a 'plastic taxon'. Warren et al. (2010) treat *altacordillera* as a junior subjective synonym of nominotypical *chryxus*.

There seems to be little justification at this time to set aside the valid lectotype of *C. chryxus* in response to the recognition of a poorly understood and contentious



taxon. The lectotype specimen has been accepted as the type of *Chionobas chryxus* for over 80 years and is still considered by the majority of lepidopterists to represent this nominal species. I therefore perceive no imminent threat to nomenclatural stability which would warrant exercising Art. 81 in accordance with Art. 75.5 of the Code.

### Supplemental remarks

I attempted by various means to locate the male specimen that was figured by Doubleday (1849). Because Edward Doubleday often exchanged specimens with the French entomologist Jean B.A.D. de Boisduval, I recently asked staff at the National Museum of Natural History (Smithsonian Institution, Washington, D.C.) to search that collection for the missing male (it was not found). With the help of library staff at NHM, I also recently searched, without success, for references to *C. chryxus* among the manuscripts of Edward Doubleday.

Scott (para. 10) perceives Article 72.4.1.1 to be subservient to Article 72.4.1, thus he will accept the lectotype as valid only if these articles are interpreted independently. According to ICZN (1999, p. XIII), each article in the current edition of the Code 'consists of one or more mandatory provisions, which are sometimes accompanied by Recommendations and/or illustrative Examples.' This establishes that all provisions must be considered, but does not indicate that subsections are universally subservient. The insertion of 72.4.1.1 into the fourth edition of Code broadens the scope of 72.4.1 and eliminates the need to designate a neotype in many instances. External evidence can be valuable when attempting to determine the type series of a nominal taxon for which there was no written description (as in *C. chryxus*). The illustration of a single specimen does not remove the possibility that the author's concept of that taxon was based upon multiple specimens. However, I feel that Article 72.4.1 lacks the necessary language to clearly embrace such circumstances, thereby resulting in confusion over the application of 72.4.1.1 (as demonstrated by Case 3495). Article 72.4 should be modified to rectify this deficiency.

Although Scott is reluctant to accept the lectotype pursuant to Article 72.4.1.1, ample published and unpublished evidence suggests that this specimen was a syntype of *C. chryxus*. In addition to the evidence reviewed by Scott, Butler (1868) did not list any other species of *Oeneis* (= *Chionobas*) in the British Museum from 'Rocky Mountains' that were available to Doubleday. Although Butler (p. 162) listed *Oeneis uhleri* (Reakirt, 1866) from 'Rocky Mountains', he restricted the locality to 'Colorado Territory' and did not denote that the British Museum possessed any specimens. This species was first collected in 1864 (Reakirt, 1866, p. 122) and the oldest specimens in NHM, from Colorado and Utah, are dated 1900 (B. Huertas, pers. comm.).

### Additional references

- Holland, R. 2010. A new subspecies of *Oeneis chryxus* (Nymphalidae: Satyridae) from south central New Mexico. *Journal of the Lepidopterists' Society*, **64**: 161–165.
- Kondla, N.G. 2010. Section 2. Butterflies. Pp. 163–192 in Pohl, G.R., Anweiler, G.G., Schmidt, B.C., & Kondla, N.G., An annotated list of the Lepidoptera of Alberta, Canada. *ZooKeys*, **38**: 1–549.
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**Comment on the proposed conservation of usage of *Testudo gigantea* Schweigger, 1812 (currently *Geochelone (Aldabrachelys) gigantea*; Reptilia, Testudines)**

(Case 3463; see BZN **66**: 34–50, 80–87, 169–186, 274–290, 352–357; **67**: 71–90, 170–178, 246–254, 319–331; **68**: 72–77)

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Hoogmoed (BZN **68**: 72–77) criticised the paper in *Zootaxa* by Frazier & Matyot (2010), calling for their conclusions to be considered void. The *Zootaxa* paper provided a detailed compilation and evaluation of numerous historic and contemporary sources, considered results of consultations with diverse colleagues, and made two fundamental conclusions: (1) the locality for RMNH 3231, the lectotype of *Testudo dussumieri* Gray, 1831, is uncertain; it is unlikely to be Aldabra Atoll, but is likely to be Mahé, granitic Seychelles; (2) the combination of apparent time and locality of collection, together with the unique haplotype, raises a possibility that the specimen is an extinct Seychelles tortoise – not an Aldabra tortoise. Despite his 6-page comment, Hoogmoed provided no new information to remove uncertainty about the provenance and taxonomic identity of the specimen, and he continues to ignore recognised sources of error. Only a brief summary of the extensive details presented in Frazier & Matyot (2010) will be given herein, where we limit the discussion to the evidential basis of the issues.

**The locality of the lectotype of *Testudo dussumieri* Gray, 1831**

There is no evidence that J.-J. Dussumier, considered to be the collector of the lectotype, ever visited Aldabra, but he is definitely known to have made collections on Mahé, in the granitic Seychelles, at a time (possibly as early as 1823) when native Seychelles tortoises were still in existence. Dussumier is also known to have visited the Mascarene Islands of Mauritius and Ile Bourbon (La Réunion), where thousands of tortoises from the granitic Seychelles, as well as from Aldabra, had been imported. For example, in his summary of historic records, Bour (1984, p. 302) reported that ‘from 1773 to 1810, at least 25 ships carrying Tortoises from central Seychelles Islands entered Mauritius’, adding in a footnote that ‘a ship could load from 500 to 6000 Tortoises’ (though the latter figure is questionable). According to Toussaint (1965, p. 56), in December 1808 the *Favorite* was still transporting a cargo of land



tortoises from Seychelles to Mauritius; likewise, in October 1807 the *Amazone* carried a load of land tortoises from Seychelles to Réunion (p. 61). A.M.C. Duméril, G. Bibron and A.H.A. Duméril, herpetologists at the Paris Museum where Dussumier's collections were received, recorded his tortoises from Anjouan (Comores) and Seychelles – there is no mention of any Dussumier tortoise from Aldabra.

Gray's original (1831) description of *T. dussumieri* is confused for many reasons; while he evidently saw a small tortoise in Leiden sometime before 1831, it is unclear – among other things – what data accompanied it, a fact recognised by Hoogmoed (BZN 68: 74). Several years later, in their section on *T. indica*, Temminck & Schlegel (1834, p. 75) included the statement 'Cet établissement a reçu du Musée de Paris un autre individu très-jeune, communiqué sous l'épithète de Test. Dussumieri, rapporté par le voyageur dont elle porte le nom, de l'île Aldebra située au nord du canal de Mosambique.' ['This institution {The Leiden Museum} has received from the Paris Museum another very young individual, sent under the name of *Test. Dussumieri*, brought by the traveler whose name it bears, from Aldabra Island situated in the north of the Mozambique Channel.']. As Hoogmoed explained (BZN 68: 74 and following pages), it is unknown on what Temminck & Schlegel based this statement.

Originally, Hoogmoed (BZN 66: 354–356; Hoogmoed et al., 2010) claimed that the lectotype has good locality data based on the assertion that the old label that accompanies RMNH 3231 was the 'original label' from Paris, but he now admits (BZN 68: 77) that 'there is a good chance that the old label' is not the original, and probably postdates both Gray and Temminck & Schlegel. He also acknowledges various other uncertainties, including unknown collection management practices during the early years of the Leiden Museum, beginning in 1820 and for the next few decades: 'About the early history of its management we know little and it even is not quite certain when the present numbering system for reptiles and amphibians jointly was started, although there are some clues to that' (p. 76). In addition, he recognises (p. 77) that 'the name *Test. dussumieri*, mentioned by Temminck & Schlegel (1834) and Gray (1831 b) does not appear in the register or on the label'. Nonetheless, he (BZN 68: 72–77) continues to defend his earlier claim that the specimen's locality is unequivocally known. The basis for his assertion now rests on Temminck & Schlegel's (1834) above-quoted statement, although Hoogmoed acknowledges that the source of this is unknown. Hoogmoed fixedly disregards, among other things, a fundamental point explained by Matyot (BZN 66: 352): there is no evidence that Dussumier ever visited Aldabra atoll, or that he provided any collections from Aldabra.

Hoogmoed's faith in the purported provenance of the specimen based on a passage made years after the original description might be understandable if there were no contradictory evidence. If Temminck & Schlegel's account were consistent with the localities reported by Dussumier and/or records of his specimens in the institution where his collections were originally received (Paris Museum), it would help build a case for the locality of the lectotype. However, Temminck & Schlegel's statement stands alone and in contrast to historic information about Dussumier's itineraries and collections. As much as Temminck & Schlegel (1834) give an authoritative account of what was known of chelonians at that time, Hoogmoed does not consider the dangers of erroneous documentation, a problem that has happened too many times in the past to be ignored – regardless of the scientific authority. This would not



be the first time Temminck's name has been associated with incorrect localities and erroneous data regarding Dussumier's travels. Desmarest (1826, pp. 215–216), reviewing Temminck's *Monographie de Mammalogies*, drew attention to several such mistakes: 'M. Temminck a été mal informé, pour l'indication des localités qu'il attribue aux animaux qu'il décrit, ou pour celle des lieux où il fait aller les voyageurs naturalistes. . . . c'est ainsi qu'il fait voyager dans ces îles {les îles Mariannes} M. Dussumier, quoique ce négociant n'y soit jamais allé. . .' ['Mr. Temminck has been misinformed regarding the localities that he attributes to the animals he describes, or the places that he claims the naturalist-travellers called at. . . it is thus that he claims Mr. Dussumier travelled to these islands {the Marianas}, whereas this merchant never went there. . .']. Moreover, Temminck is known to have made other serious mistakes in specimen localities and other associated data, some of which were described by Chris Smeenk, former Curator of Mammals of the Leiden Museum. Smeenk (2009) did a detailed evaluation of historic and bibliographic information concerning one of Captain Cook's Australian possums *Pseudocheirus peregrinus* (Boddaert, 1785) and he stated flatly (p. 733): 'Temminck (1824) has added to the confusion', explaining several errors and the evident confounding of collectors and localities by this 19th century ornithologist. Smeenk's summary remark (p. 737) is critical: 'In this connection, it should be emphasized that many, if not most, early specimens in the Leiden Museum are insufficiently documented.'

Were RMNH 3231 just any specimen, the uncertainty about the locality might not be so important, but this is a lectotype, designated by opponents of Case 3463 to be the name-bearing type of the Aldabra tortoise. It hardly needs explaining further the tremendous, and unnecessary, confusion that would be caused by using a name-bearing type that has an uncertain provenance – worse yet if it turned out to have a locality totally inappropriate to the taxon in question. Myriad biological studies have faced serious problems for having relied on erroneous specimen documentation (e.g. Rasmussen & Prys-Jones, 2003; Boessenkool et al., 2010).

As Dunn & Stuart (1951, p. 677) eloquently explained: 'Just as reexamination of a type specimen may bring to light errors in the original description or characters not mentioned in it, so reexamination of the data accompanying the type specimen or related to it (original labels, collector's notes, or itineraries, etc.) may add precision to or even alter the type locality as given in the original description.' Article 76.2 of the Code makes it very clear that the precise locality of a lectotype is determined by the place of origin, not necessarily previously published statements. More and more speculations about what might, or might not, have happened to RMNH 3231 will not turn an equivocal locality into a known fact.

### **The identity of the lectotype of *Testudo dussumieri* Gray, 1831**

With the evidence that Dussumier's tortoise was most likely collected in the granitic Seychelles at a time when the native tortoises were still extant, or possibly in the Mascarene Islands to where both Seychelles and Aldabra tortoises had been shipped, its taxonomic identity cannot be assumed. To date, no one who has declared that the lectotype is an Aldabra tortoise has provided a single basic measurement, much less a description of the diagnostic characters used to distinguish it from extinct Seychelles tortoise taxa.



Austin et al. (2003) have done the only genetic study on RMNH 3231, based on a 336-bp fragment of mtDNA. They reported this specimen as haplotype B, with two nucleotide substitutions from the common haplotype A of the Aldabra tortoise; out of the 37 non-Madagascan specimens on which they reported, RMNH 3231 has a unique haplotype. Although this does not prove that the lectotype is from a different lineage, it contrasts with the lack of genetic variation in 915-pb fragments of mtDNA that Balmer et al. (2010) found in a sample of 112 tortoises on Aldabra. Notably, while Austin et al. concluded (p. 1422), with very careful language, that ‘the mtDNA of non-Madagascan *Aldabrachelys* studied here suggests that only a single species may be involved’ they preceded (p. 1421) this with the caveat: ‘there may have been some sampling of extinct lineages.’ Aware that information on genetic diversity of western Indian Ocean tortoise populations – particularly the extinct granitic Seychelles lineage(s) – is poorly known, Austin et al. were cautious about over-extending the interpretation of their results and making dogmatic statements. Contrary to Hoogmoed’s claim that the genetic research proves that the lectotype is an Aldabra tortoise, what is known to date of non-Madagascan *Aldabrachelys* haplotypes is not sufficient for distinguishing closely related lineages or specimen provenance (Austin in litt. 27 April 2011). Hence, the taxonomic identity of the lectotype remains unresolved.

## Conclusion

The absence of an unequivocal locality defeats the supposed scientific value of RMNH 3231 as a name-bearing type for the Aldabra tortoise. Taken together with the uncertain taxonomic identity, the designation of this specimen as the name-bearing type for the Aldabra tortoise, and the continued use of the binomen, would only encourage debate, discord, and nomenclatural instability, incompatible with the primary objective of the Code: nomenclatural stability and universality.

## Additional references

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**Comment on the proposed conservation of the specific name of *Cyclodina aenea* Girard, 1857 (currently *Oligosoma aeneum*; Reptilia, Squamata, SCINCIDAE) and suppression of the senior subjective synonym *Tiliqua ornata* Gray, 1843 (currently *Oligosoma ornatum*)**

(Case 3510; see BZN 67: 307–313)

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Although our case proposed the suppression of the name *Tiliqua ornata* Gray, 1843, in the light of subsequent discussions we have decided that the most important objective is to conserve the current usage of the names of the two skinks in question and that this could best be achieved by using a different approach. If a specimen of the ornate skink were to be designated as neotype under Article 75.6 of the Code to replace the holotype of *Oligosoma ornatum* (which is, in fact, a copper skink) then the current usage of the names of both *Oligosoma ornatum* and *O. aeneum* would be conserved.

We consider that the specimen referred to as a ‘homotype’ (possibly a misspelling of homeotype?) by Hardy (1977) for *Cyclodina ornata* – NMNZ RE.002457 (formerly NMNZ R.1815 and ED S.912), collected at Manakau, Horowhenua, by A.H. Whitaker, 7 September 1971, is indeed an example of the ornate skink and would therefore be a suitable neotype, thus conserving the current usage of both the scientific and vernacular names for this taxon.

Consequently we would like to withdraw our previous proposal and, instead, the International Commission on Zoological Nomenclature is accordingly asked:

- (1) to set aside the existing holotype of *Tiliqua ornata* Gray, 1843 and to designate as neotype specimen NMNZ RE.002457 (formerly NMNZ R.1815 and ED S.912) in the National Museum of New Zealand, Wellington, collected at Manakau, Horowhenua, by A.H. Whitaker, 7 September 1971;
- (2) to place on the Official List of Specific Names in Zoology the name *ornata* Gray, 1843, as published in the binomen *Tiliqua ornata*, and as defined by the neotype designated in (1) above.

## Erratum

The heading in BZN 67: 326 that reads ‘People who support summary comment on Case 3463 (list compiled between 14 and 3 November 2010)’ should read: ‘People who support the above summary comment on Case 3463 (list compiled between 14 October and 3 November 2010)’.